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Dr. Joshua Lederberg
Department of Genetics
The University of Wisconsin
Madison 6, Wisconsin

Dear Dr. Lederberg:

Thank you for your reading of the manuscript and for the suggestions. I have revised the paper on the basis of your suggestions and those of the editorial staff of the Journal of Bacteriology. On the question of terminology I prefer for the present to retain the term conversion for the diphtheria phenomenon. Until the fundamental relationship of these diverse genetic phenomena become clarified I feel there is some merit in underlining their differences.

In some recent work evidence has appeared which indicates that the ability of phage beta to toxigenize is a characteristicx capable of segregating in an independent fashion from other traits eg host range. Strain C4 lysogenized with the non-toxigenizing phage is still sensitive to beta phage. Organisms carrying what appear to be recombinant phages can be isolated from resistant growth following beta lysis. Some strains are non-toxigenic yet carry a phage identical to beta in other characteristics. On the other hand some are toxigenic yet the phage being carried reflects for example the host range of the original non-toxigenizing phage. It is this phase of the work that we are currently working on. Unfortunately teaching duties and loss of my technician through marriage have slowed up the effort.

While the above work pin points the critical site of activity in the prophage to an even smaller unit I am in agreement with your observation that we are still no closer to the basic mechanism of conversion. I am of the opinion that for the present there is no way of frontally attacking the problem which resolves itself to one of nuclear-cytoplasmic interactions. Nevertheless the relationship of phage genetic material to that of the host is an intriguing problem in itself.

Again my thanks for your time and I hope to continue this correspondence with somewhat greater regularity than in the past.

Sincerely yours,

Neal B. Groman

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